for 1866, 1867, 1868, Prof. Agassiz[1] had already called attention to the probable great antiquity of the oceanic basins.

Dr. Carpenter seems also to have overlooked the series of physical observations of the depths of the sea commenced by the United States Coast Survey[2] in 1850, and carried on without interruption to the present day.

The statement made by Mr. Wild[3] that the deepest sounding of the *Tuscarora* is not trustworthy, because "no sample of the bottom was brought up," is apparently endorsed by Dr. Carpenter, who says: "The sounding wire of the United States ship *Tuscarora* twice broke without reaching bottom . . . . at depths considerably exceeding 4,000 fathoms." This should be modified by stating that the *wire broke while reeling in* twice, once the bottom was not reached, and five casts were made over 4,000 fathoms, bringing up each time a specimen of the bottom.[4] Capt. Geo. E. Belknap, of the *Tuscarora*, says,[5] speaking of the casts beyond 4,000 fathoms in depth: "The wire parted at the last two and deepest casts. . . . the result of momentary carelessness on the part of the men at the *reeling-in wheel*."

The method of sounding with wire has now been in use long enough to show that even if the *Tuscarora* had not brought up a single specimen of the bottom during her whole trip, and if the wire had *invariably broken while reeling in*, we could not for that reason alone have rejected those soundings as inaccurate.

Those who have sounded with wire know that the instant the sinker has touched bottom is recorded on deck, and the precise depth is then known, whether the cylinder is brought up or not. There is no more reason for rejecting the deepest sounding of the *Tuscarora* of 4,655 fathoms than for rejecting the 480 other casts which are accepted because a bottom specimen came up.

Cambridge, Mass., April 5 ALEXANDER AGASSIZ

[1] *Bulletin* of the Museum of Comp. Zoology, 1869, vol. i., No. 13.

[2] Coast Survey Reports, 1850 to present day; also Bibliography of Biological Results (*Bull.* Mus. Comp. Zool., vol. v., No. 9, 1878).

[3] "Thalassa," 1877, p. 15.

[4] "Deep-Sea Soundings in the North Pacific obtained by the United States ship *Tuscarora*" (Washington: Hydrographic Office, 1874, No. 54, p. 30):—

| 1874. | | Fathoms. | | |
|---|---|---|---|---|
| June 11 | ... | 4,643 | ... | Wire broke; *bottom not reached.* |
| ,, 17 | ... | 4,340 | ... | Yellow and clay brown mud. |
| ,, 17 | ... | 4,356 | ... | Yellowish mud and sand and specks of lava. |
| ,, 18 | ... | 4,041 | ... | Yellow and clay-coloured mud and gravel. |
| ,, 18 | ... | 4,234 | ... | Rocky; point of cylinder came up battered. |
| ,, 18 | ... | 4,120 | ... | Yellow and clay-coloured mud mixed. |
| ,, 18 | ... | 4,411 | ... | No specimen; wire broke (*while reeling in*). |
| ,, 19 | ... | 4,655 | ... | ,, ,, ,, ,, |

[5] *United Service Magazine*, July, 1879.

## On the Alum Bay Flora

IN the list of fossils appended to the paper upon the Alum Bay flora, brought before the Royal Society by Baron von Ettingsbausen and reported in NATURE, vol. xxi. p. 555, the new species have Ett. and Gard. attached to them, implying that Ettingshausen and myself are their authors. It is only fair to Ettingshausen to state that I had no share in making the determinations, and to myself, that I accept them simply as provisional. Associated as he is with me in the work upon the British eocene floras, he felt that he could hardly publish preliminary work connected with it in any other way. I completely disagree with him, however, as to the utility of publishing new specific names unaccompanied by drawings or descriptions of any kind, and think that a simple list of genera, with the number of new species in each, would have been unattended with any inconvenience. He appears to me to attach altogether undue weight to mere priority in nomenclature, and the existence of such provisional lists, far from aiding research, must prove a serious difficulty to our fellow workers. In the highly probable event of an author being unable to come from some distant country to examine the specimens themselves, is he, for instance, to forbear naming every undescribed species of such common Tertiary genera as *Ficus*, of which eight new and unpublished species are in the list, of *Celastrus*, of which there are five, or of any other of the some fifty genera containing new specific names? He could not safely name even any indeterminable leaf or fruit, for fear it might be one of the long list of Phyllites or Carpolithes for which Ettingshausen has devised specific names.

But were our supposititious author to go on with his work, in spite of this "sword of Damocles," would Baron Ettingshausen claim priority and deprive the man who had first figured and *published* descriptions of them, of the pleasure of christening them in accordance with his views and wishes? If not, *cui bono?*

To show the purely provisional light in which the list must be regarded, I may mention that, unfortunately just as the Baron left England, a large collection, that of the late M. Watelet from the Grès du Soissonnais, came into my possession, and seems, on a cursory examination, to contain a preponderance of species identical with those of Alum Bay. None of Watelet's published species appear in the list of the Alum Bay flora, which therefore must of necessity be considerably modified to include them. The same may be said of the flora of Gelinden, of which a large series has also reached me.

Again, even in the only section of plants yet worked out by us for the palæontographical memoir, the ferns, discrepancies occur. Two ferns occur in this Alum Bay list which are not included in our fern flora from that locality. These are inserted on the authority of Heer, who states that he has seen them from Alum Bay; but as on the occasion of that gentleman's visit or visits to England many years ago the floras from the different localities had not been systematically collected, and were generally mixed together in museums, in the same drawers and cases, and cannot always be identified by the matrix, I prefer to adhere to the opinion of that indefatigable collector, Henry Keeping, who lived within a short distance of Alum Bay, and to my own, Mr. Mitchell's, and all other workers' experience, that no fern but *Marattia* is found there. At all events, if they are to be included in the Alum Bay flora, they should be so with reserve, especially as Prof. Heer's ideas as to the position of the localities and their ages are so hazy that he puts the Alum Bay leaves in the "Bartonisem" (above, if anything), or about 1,000 feet too high, and thinks that Bournemouth is somewhere in the Isle of Wight.

An illustration of the inconvenience caused by publishing names without proper figures and descriptions occurs to me. Heer named a small fern fragment which he supposed to be from Alum Bay, *Asplenium martinsi*. This name has got into works by Saporta and Crié, who have each tried to fit ferns of their own into Heer's meagre description. Neither had seen the original, nor could they give any information, and it was only after several attempts to obtain it that Ettingshausen received a rough sketch from Heer showing conclusively that the "species" in question was a fragment of the abundant and well-known *Anemia subcretacea* of Sézanne. I do not even now know whether it was upon this fragment or some other that Heer wrote that he had "seen this form" (*Anemia subcretacea*) from Alum Bay.

J. STARKIE GARDNER

## Negritoes in Borneo

HAVING had inquiries addressed to me as to the existence of a Negrito race in Borneo, I think it may be useful to recall attention to, and possibly save from oblivion, a statement on this subject which was published by Windsor Earl in the *Journal* of the East Indian Archipelago. Mr. Earl says that a Capt. Brownrigg, who had been shipwrecked on the east coast of Borneo, informed him (*J.E.I.A.*, No. 9) that he had lived several months at a town some distance up the Berau River, and that during his stay the town was once visited by a small party of men from the interior, "who must have been of the Papuan race" (*sic*). He described them as being short, strongly-built people, black in complexion, with hair so short and curly that the head appeared to be covered with little knobs like peas; and with many raised scarifications over the breast and shoulders. He described them as being on good terms with the people of the town, mostly Bugis, and as supplying them occasionally with jungle produce.

Of this account it may be remarked that Mr. Earl would not have retailed it unless he had had some confidence in the credibility of his informant—that, so far as it goes, it is curiously circumstantial—and that these people are said to have come exactly from that district in Borneo where we might expect *à priori* to find Negritoes if they existed at all.

Whilst on the subject of Borneo, may I suggest that ethnologists should make a more sparing use of the term "Dyak" when treating of the Malay Archipelago? It should only be applied to tribes who themselves use it as the distinctive appellation of their people. As more than one tribe so uses it, there should always be prefixed some word still further limiting its applica-

tion in each particular case. As employed by Malays, who are followed both by Dutch and English travellers, the word has scarcely better standing-ground in a scientific terminology than has "Alfuro."

The following fact with regard to the Sea Dyaks may be of interest. When Europeans first entered Sarawak the Kayans, properly so called, were dominant in the great Rejang River, and the Sea-Dyaks were strictly confined to the Batang Lupar, Saribas, and Kalakah rivers. Now the Sea-Dyak population of the Rejang is some 30,000, and the Rejang Dyaks are rapidly occupying the Oyah, Mukah, and Tatau rivers further up coast. On the original Sea-Dyak rivers the people always use the expression "we Dyaks" when they mention their own race; but on the Rejang the expression "we Iban" will invariably be heard—the explanation being that the Kayans habitually designate Sea-Dyaks as "Ivan" among themselves, whence the Dyaks have applied the name; but having no v-sound in their language, they say "Iban." The Kayan proper is rich in v-sounds. I have been informed, though I cannot vouch for the accuracy of the statement, that "Ivan" in Kayan is a term carrying with it a sense of opprobrium. However this may be, it is remarkable that so large a section of the Sea-Dyaks, who are so thoroughly dominant in Rejang, and are in constant daily communication with their original seat in the rivers to the westward, should in the course of some thirty years have come to habitually speak of themselves by the name given them by their foes. And it is the more surprising because the Sea-Dyaks generally give new names of their own to the geographical features of the district into which they immigrate.

Papar, North Borneo A. HART EVERETT

## Seeing by Electricity

WE hear that a sealed account of an invention for seeing by telegraphy has been deposited by the inventor of the telephone. Whilst we are still quite in ignorance of the nature of this invention, it may be well to intimate that complete means for seeing by telegraphy have been known for some time by scientific men. The following plan has often been discussed by us with our friends, and, no doubt, has suggested itself to others acquainted with the physical discoveries of the last four years. It has not been carried out because of its elaborate nature, and on account of its expensive character, nor should we recommend its being carried out in this form. But if the new American invention, to which reference has been made, should turn out to be some plan of this kind, then this letter may do good in preventing monopoly in an invention which really is the joint property of Willoughby Smith, Sabine, and other scientific men, rather than of a particular man who has had sufficient money and leisure to carry out the idea. The plan, which was suggested to us some three years ago more immediately by a picture in *Punch*, and governed by Willoughby Smith's experiments, was this:—Our transmitter at A consisted of a large surface made up of very small separate squares of selenium. One end of each piece was connected by an insulated wire with the distant place, B, and the other end of each piece connected with the ground, in accordance with the plan commonly employed with telegraph instruments. The object whose image was to be sent by telegraph was illuminated very strongly, and, by means of a lens, a very large image thrown on the surface of the transmitter. Now it is well known that if each little piece of selenium forms part of a circuit in which there is a constant electromotive force, say of a Voltaic battery, the current passing through each piece will depend on its illumination. Hence the strength of electric current in each telegraph line would depend on the illumination of its extremity. Our *receiver* at the distant place, B, was, in our original plan, a collection of magnetic needles, the position of each of which (as in the ordinary needle telegraph) was controlled by the electric current passing through the particular telegraph wire with which it was connected. Each magnet, by its movement, closed or opened an aperture through which light passed to illuminate the back of a small square of frosted glass. There were, of course, as many of these illuminated squares at B as of selenium squares at A, and it is quite evident that since the illumination of each square depends on the strength of the current in its circuit, and this current depends on the illumination of the selenium at the other end of the wire, the image of a distant object would in this way be transmitted as a mosaic by electricity.

A more promising arrangement, suggested by Prof. Kerr's experiments, consisted in having each little square at B made of silvered soft iron, and forming the end of the core round which the corresponding current passed. The surface formed by these squares at B was to be illuminated by a great beam of light polarised by reflection from glass, and received again by an analyser. It is evident that, since the intensity of the analysed light depends on the rotation of the plane of polarisation by each little square of iron, and since this depends on the strength of the current, and that again on the illumination of the selenium, we have another method of receiving at B the illumination of the little square at A. It is probable that Prof. Graham Bell's description may relate to some plan of a much simpler kind than either of ours; but in any case it is well to show that the discovery of the light effect on selenium carries with it the principle of a plan for seeing by electricity. JOHN PERRY

Scientific Club, April 21 W. E. AYRTON

## Musical Sounds within the Ear

I SHOULD like to know how far the musical sounds, which we sometimes hear within our ears, are of different pitch in different persons. From repeated observations I find that my left ear gives G, and the right one B. A friend of mine, who is a good performer on the violin, finds F and A respectively.

It is perhaps not without interest that in some parts of Germany (at least in Silesia) people believe these sounds to be indicative of one's being talked about, and that the sound ceases as soon as one thinks of the person who is supposed to do so.

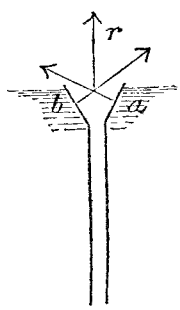
Caráca3, March 18 A. ERNST

## Ice Filaments

"THE comb-shaped masses of ice of fibrous structure" mentioned by your correspondent, in explanation of the inquiry made by the Duke of Argyll, are observed every winter in the southern portion of the United States, especially on the sloping sides of a path or country road where the surface-earth has been removed, and the natural clay sub-soil is not rendered compact by being trodden. The conditions requisite for its abundant production are a sudden reduction of temperature below the freezing-point when the clay soil is thoroughly saturated with water. When this occurs at sunset, the next morning, if the night continues favourable, will disclose a vast collection of fibrous filaments, from two to six inches in height, rising from the soil in close juxtaposition, generally holding aloft in their caps portions of the soil, the longest crystals appearing when the soil is free from surface-loam.

I have frequently given to my class this explanation of the phenomena.

The capillary tubes of the soil are all filled up to the surface with water. The sudden reduction of temperature freezes the water at the *surface*, but does not chill it within the soil below 32° F. The consequence is that this expansion, caused by congelation at the upper extremity of the capillary tube, compresses the walls of the tube externally, and causes the mouth of the tube at the surface to assume a conical shape, as in diagram. The congelation of all the water within the conical cavity causes pressure normal to the surface of the cone at *a* and *b*, and hence produces a vertical resultant, *r*, that raises the cone of ice. Capillary action immediately fills the little cavity with water, which in turn is frozen and elevated by the expansion force of its congelation. The filament thus grows in this simple way from its base. The soil in which these fibrous crys als or filaments form is never frozen; thus proving the correctness of the explanation.




They are formed very rapidly. I have on more than one occasion, when a sudden chill at sunset would start them growing, listened to the crackling of the little ice-crystals as they would break loose from each other, being pushed up by this expansive force.

I infer the filaments of ice formed on rotten wood are due to a similar cause, and that they will not be formed unless the reduction of temperature is quite sudden. That is, if the reduction of temperature is so gradual that the water somewhat below the surface in the cylindrical portion of the capillary tube is frozen, the crystals will not be elevated, but the ground will be frozen. WM. LEROY BROUN

Vanderbill University, U.S.